14 Mar 1991, 10:30 am - 12:30 pm

Session 3: Opening Remarks

G. Castro

OPENING REMARKS
By: G. Castro

This discussion will address five of the six questions raised by the General Reporter for Session III, Dr. Richard Campanella. The questions are not repeated below, and the reader is referred to the General Report.

Question 1 deals with the type of samples, remolded or undisturbed, that should be used in laboratory investigations. The testing of remolded samples is appropriate for research on the effect of parameters influencing the development of liquefaction, for which the testing of identical specimens under various conditions is desirable. On the other hand, undisturbed samples are essential for the investigation of a specific site, not only for testing but also for observation of the character of the soils, fabric and stratification. The results of testing of remolded samples can be grossly misleading when attempting to predict in situ behavior of an actual natural or manmade soil deposit. Of course, undisturbed samples are never truly undisturbed, and interpretation of the test results must consider changes in void ratio and other results of disturbance.

Question 2 relates to a comparison of the usefulness of SPT, DMT, and CPT for use in empirical liquefaction correlations. The characterization of a soil deposit requires a combination of field procedures, and it is not possible to rank their usefulness. However, the main focus of the question relates to the use of empirical charts (e.g., Seed et al.) for predicting liquefaction. These charts present actual case histories of observations of manifestation of high pore pressures, mostly sand boils, or absence of them, as a result of actual earthquakes. Present charts use one value of SPT to describe in full the soil deposit. The question is whether CPT or DMT values may be better alternatives to the SPT value, and thus whether we should encourage their use at earthquake sites. The empirical charts are a crude tool, since development of pore pressure and sand boils is a function of the properties of the full soil profile rather than of a single blowcount in a specific layer. Use of other in situ tests for this purpose will not eliminate the inherent crudeness of the procedure. However, as noted above, DMT and CPT may be desirable from an overall soil characterization standpoint.

Question 3 deals with the use of shear wave velocity measurements in liquefaction investigations, and an apparent discrepancy on the influence of fines on liquefaction, as inferred from V_s and blowcount data. Shear wave velocity is an engineering property, while the SPT is an index test, which has value only to the degree that one can correlate it to the engineering properties. There is substantial evidence that seismic soil behavior is well correlated with shear strain, which in turn is primarily a function of V_s. Thus V_s has a direct application in the analysis of soil behavior and should be measured as part of all but the most crude evaluation of liquefaction. The apparent discrepancy referred to by the Reporter is that on one hand, at equal blowcount, V_s of sands does not appear to be a function of fines content, while on the other hand, the empirical chart based on blowcounts indicate that, for soils with the same blowcount, there is less likelihood of sand boils for the sands with more fines. In this discussor's opinion there is no discrepancy. The occurrence of sand boils requires a certain distribution of permeability with depth, i.e., that pore pressures are generated in a soil layer overlain by a less pervious soil. When pore pressures are generated in a soil layer, reconsolidation will cause an upward flow of water. This upward flow cannot cause sand boils if the overlying soil is more pervious, e.g., a clean sand overlying a loose silty sand layer in which pore pressures are generated. Thus one should not reach conclusions related to the effect of fines <u>on pore pressure generation</u> from the empirical charts.

In Question 4 the Reporter is asking for a comparison of methods to estimate in situ steady state strength S_{us}, namely laboratory tests based on Poulos et al., and the use of an empirical correlation with blowcount. Dobry's method of determining a ratio of S_{us} to consolidation pressure was also included in the question. The Reporter noted that there is disparity between the results of laboratory measured S_{us} value and values backfigured from actual failures. This discussor strongly disagrees. The only actual failure for which values of S_{us} have been measured is the Lower San Fernando Dam, and they were in excellent agreement with the observed failure mechanism. Obviously more cases are needed. The implicit assumption that blowcounts and S_{us} can be uniquely correlated is not warranted. The soil behavior during SPT tests is at least partly drained, while S_{us} is a fully undrained property. The S_{us} to consolidation stress ratio in Dobry's method is based on laboratory tests in which one attempts to reproduce in the laboratory the depositional environment of the in situ soils. If this is successfully accomplished then the results have direct application in practice. Dobry's RDWPSS sample preparation method applies to silty sands and probably provides a lower bound for alluvial deposits. This may not be the case for clean sands.

Question 5 deals with whether there are reliable procedures to estimate lateral spreading deformations. This discussor feels that there are such procedures and that the key to their us is the proper determination of the operational strength during the movements, i.e., the undrained steady state strength in saturated loose sands. See for example an analysis of the Heber Road lateral spreading in Castro, 1987. The reader is referred to the General Report for Session VII by Larry Von Thun for a good summary of procedures presenting in this Conference for estimating deformations.